ANALYSIS

Edited by Bernard Mayo, with the advice of A. J. Ayer, R. B. Braithwaite, Herbert Dingle, A. E. Duncan-Jones, P. T. Geach, C. A. Mace, A. M. MacIver, and H. H. Price

Volume 21
1960–1961

BASIL BLACKWELL · BROAD STREET · OXFORD

CONTENTS

CONTENTS

Vol. 21 No. 1 (New Series No. 79) October 1960

ANALYSIS

CONTENTS

THREE SHILLINGS AND SIXPENCE NET

BASIL BLACKWELL · BROAD STREET · OXFORD

ACTS AND RELATIONS IN BRENTANO[1]

By REINHARDT GROSSMANN

THE sentence 'I think of Paris' seems to mention three things, namely, a self, an act of thinking, and Paris. I say seems because there has of course always been considerable disagreement among philosophers as to the existence of selves, acts, and physical objects. According to what I shall call the classical view on mental acts, however, 'I think of Paris' does indeed mention a mental act of thinking. According to this view, *mental acts exist*. This is its first main thesis. There are two further ones.

Philosophers are generally not satisfied with the mere assertion that things *exist*. They wish to know what kind of thing it is that is said to exist. In the most general case, they wish to know whether a given existent is either an individual (particular), or a property (universal), or, perhaps, a relation. Usually, the classification offers no particular difficulties. It does, as we shall see, in the case of mental acts. The classical view, though, is quite definite and explicit on this point. Take again the paradigm 'I think of Paris' and consider for comparison the sentence 'A is greater than B'. It, too, seems to mention three things, namely, A, B, and the relation of *being greater than*. This similarity between statements about acts and statements about two-term relations is rather striking. In the tradition it was considered to be more than a similarity. *Acts*, one maintained, *are relations*. This is the second thesis of the classical view.

One could draw a diagram of a statement about an act, the self as well as the object of the act being represented by circles. The act itself, however, could not possibly be symbolized by a third circle. Its representation must (1) somehow *connect* the self and the object; this is its relational feature; and (2) somehow show a *direction* from the self to the object. Both requirements are met if one draws an arrow from the circle that represents the self to the circle that represents the object. This completes the diagram. It shows that acts always have a direction. Acts are said to 'intend' their objects. In brief, *acts are intentional*. This is the third thesis.

Brentano, while examining the classical view, noticed a problem connected with the thesis that acts are relations. For a relation to obtain between two things, there must be two things. Similarly, in terms of the diagram, for an arrow to point at an object, there must be an object for the arrow to point at. When I think, for instance, of Paris, my thinking

[1] Read at the meeting of the American Philosophical Association at Madison, Wisconsin, 1959.

may be said to intend the object Paris. But now there are also cases where acts intend 'objects which do not exist'. When I think of Pegasus, for instance, the so-called object of my thinking seems to be Pegasus. Yet Pegasus, as every one will agree, does not exist. Hence there is apparently only one existent in this case, namely, the self; and there is, consequently, nothing for the arrow to point at. The difficulty, then, with the second thesis of the classical view on acts is that acts must sometimes be said to intend objects which do not exist.

Faced with this difficulty, some philosophers concluded that there must be objects which do not exist.[1] Brentano, however, could not bring himself to accept non-existing objects. He concluded instead that acts cannot be relations and hence repudiated the second thesis of the classical view. But this conclusion had an equally unacceptable implication. If acts are not relations, no connection is left between a self and the rest of the world. It then remains to be shown how knowledge of so-called independent physical objects is possible. Brentano, being aware of this implication of the categorical rejection of the second thesis, tried to show that acts, not being relations, nevertheless are in some sense relational. I shall argue (1) that his acts cannot possibly be relational in the usual sense, (2) that what he means by 'relational' in this context is a psychological rather than an ontological characteristic of acts, and (3) that he does not therefore succeed in solving the problem of how selves could possibly be connected with other things through non-relational acts.

Brentano's solution of the problem of non-existing objects has two parts. First: he reduces the number of constituents in an act situation to two, namely, a self and a property of a self.[2] 'I think of Paris' is therefore in his opinion not a statement about a self, an act of thinking, and Paris, as the classical view has it, but, rather, a statement about a self and a property of it, namely, the property referred to by 'thinking of Paris'. *Mental acts are not relations, but properties* (Akzidentien) *of selves.* Call these properties act-properties; the corresponding predicates, act-predicates. Second: Pegasus, we know, does not exist. Brentano therefore concludes that the word 'Pegasus' could not refer to anything, neither when it stands alone, nor when it occurs in such an act-predicate as 'thinking of Pegasus'. He asserts, moreover, that no word or expression that purports to refer to an object in an act-predicate really refers to anything. In general, always according to Brentano, 'I think of an object' merely *seems* to mention an object. Actually, the word 'object'

[1] Compare, for instance, A. Meinong, *Ueber Annahmen* (Second edition), Leipzig, 1910; also K. Twardowski, *Zur Lehre vom Inhalt und Gegenstand der Vorstellungen*, Vienna, 1894.

[2] It must be mentioned that Brentano is a nominalist. He knows, therefore, no properties in the ordinary sense. When he speaks of 'Akzidentien', he means something very much out of the ordinary, something that requires a detailed explanation. For the point of this paper, however, it makes no difference whether we think of Brentano's acts as 'Akzidentien' in his very special sense or simply as properties in the ordinary sense.

is not a name at all. This is the second part of his solution. It is clear, I think, how this method of introducing act-predicates solves the problem of non-existing objects. No longer need Brentano be worried by the fact that one can think of Pegasus when there is no Pegasus. On his analysis, 'I think of Pegasus' does not mention Pegasus at all. Properly analysed, this sentence becomes a statement about a self and one of its act-properties.

Let us accept Brentano's analysis for the sake of the argument and ask whether, having accepted it, one can still hold that acts are intentional. This inquiry will prepare us for the discussion of Brentano's assertion that act-properties are relational.

If one means by 'intentional' what according to the classical view can only be expressed by an arrow, then the answer is obvious at once. Act-properties could not possibly be intentional in this sense. I can, however, think of one other sense of 'intentional'. It might be the case that Brentano's transcriptions of statements about acts yield so-called *intensional* contexts. If so, than one could say that his acts are intentional inasmuch as every properly analysed statement about an act will turn out to be an intensional context. A criterion for intentionality in this sense is readily available. It makes use of the following feature a language L may or may not possess. Two expressions (of L) referring to the same thing are interchangeable in a given context (of L) *salva veritate*. This is the so-called principle of substitutivity (PS).[1] A context of L is said to be intensional if and only if PS does not hold for it. Otherwise it is non-intensional. This is the criterion. L is said to be intensional if and only if it contains at least one context for which PS does not hold. English, for instance, is considered to be intensional. To see this, consider that the two expressions 'the morning star' and 'the evening star' refer to the same thing and that substitution of the former for the latter may lead from the true sentence 'John believes that the evening star is identical with the evening star' to the false sentence 'John believes that the morning star is identical with the evening star'.

Brentano transcribes 'I think of Paris' into 'I am thinking-of-Paris', where 'thinking-of-Paris' is construed as an act-predicate. Does PS hold for this transcription or not? One may easily be misled into thinking that it does not and therefore conclude that Brentano's transcriptions are intensional; for one may erroneously reason as follows. 'Paris' and 'the capital of France' refer to the same thing. Substitution of 'the capital of France' for 'Paris' in 'I am thinking of Paris' yields 'I am thinking of the capital of France'. But 'I am thinking of Paris' may be true, while 'I am thinking of the capital of France' is false, and conversely. Hence, 'I am thinking of Paris' is intensional. To see that this conclusion is false, remember that according to Brentano 'Paris' is

[1] Compare R. Carnap, *Meaning and Necessity*; and Quine's *From a Logical Point of View*.

not a genuine constituent of 'I am thinking-of-Paris'. It is in this context, as he sometimes says, a synsemantic expression. As such, it does not refer to anything and could therefore not possibly refer to the same thing as 'the capital of France'. Brentano's view simply prohibits substitution in act-predicates. 'Thinking-of-Paris' is one indivisible predicate for which the question of substitution cannot even arise. I conclude that his transcriptions of statements about acts are not intentional in the sense under consideration. Nor are they intentional in any other sense I can think of.

We saw that Brentano rejects the classical view that acts are relations. Mental acts, he holds, are properties (Akzidentien). We must now ask what Brentano has in mind when he goes on to describe act-properties as being relational.

Consider the sentence 'Titus is taller than Caius' and the relational predicate 'taller than Caius'. If one treats *taller than* as a relation between two existents, and if one derives the relational property *taller than Caius* from this relation, then it may happen that the property *taller than Caius* ceases to be predicable of Titus, even if Titus should not have changed at all. This would be so, for instance, if Caius grew to be taller than Titus. This shows, according to Brentano, that the notion of a relational property (Relativum) must not be derived from the notion of a relation that can only obtain between two existents. On the contrary, the notion of a relational property must be taken as the fundamental one. And one must also realize, always according to Brentano, that the predication of a relational property does not necessarily involve that the corresponding relation holds between two existents. For example, in order that the relational property *taller than Caius* be predicable of Titus, it suffices to think of Caius as being of a certain height; whether Caius actually exists or not and whether he actually is of this or that height makes no difference whatsoever. It follows then that this property will not cease to be predicable of Titus through any change in Caius. It could only cease to be predicable of Titus, if Titus himself changes in height; for what one predicates of Titus when one ascribes to him the relational property of being taller than Caius is nothing but the property of being taller than Caius is thought or imagined to be.

Having done away with relations and put in their place relational properties, Brentano explains that acts are relational or, more accurately, that they are relational properties. Remember that to predicate of anything a relational property does not presuppose that both constituents of the corresponding relation exist. Hence, to say that a self has the relational property of thinking of Pegasus does not presuppose the existence of Pegasus. Brentano, it seems, has succeeded in solving the problem of non-existing objects without losing the relational characteristic of acts. But his success is only apparent.

'Thinking of Pegasus', I emphasized earlier, is for Brentano one indivisible predicate. So is, as we just saw, 'being taller than Caius'. Replace these two predicates by 'N' and 'M', respectively. What distinguishes now the relational properties N and M from non-relational ones? Brentano, I think, foresaw the question. This is to his credit. His answer, however, is not; for it shows clearly that, contrary to his own belief, Brentano's acts are not relational. He replies that whenever one thinks of Titus as being taller than Caius or of John as thinking of Paris, then there occur two acts of thinking. In *modus rectus*, one thinks in the first case of Titus; in the second, of John. In *modus obliquus*, however, one also thinks of something else: in the first case of Caius; in the second, of Paris. Generally, whenever one thinks of a relational property, there occur two acts of thinking, one in *modus rectus*, the other in *modus obliquus*. And whenever one thinks of a non-relational property, the thinking in *modus obliquus* (in this sense) is absent. This is Brentano's distinction between relational and non-relational properties. On this distinction alone rests the assertion that act-properties are relational. But this is to say that acts are relational because one thinks of them in a certain way or, rather, that acts are really properties, properties, however, of which one thinks differently from other properties. The different way of thinking of act-properties is then called their relational feature.

Notice, though, that the acts of thinking in *modus rectus* and *modus obliquus* are themselves act-properties. When I think of John as thinking of Paris, there is therefore still only one mental substance which is now modified in two ways, and not, as one may perhaps think, two arrows pointing at John and Paris, respectively: one, if I may so put it, pointing from myself to John thinking of Paris, the other, from John's self to Paris. The only difference between thinking of a relational property and thinking of a non-relational property consists in the fact that in the former case the thinker's self is modified in two ways, while in the latter there is only one.

But whether acts are relational or not in the usual sense, does not at all depend on how we think of them. Brentano, it seems, confuses a philosophical question, namely, the nature of relations, with a psychological one, namely, how we think about them; or, if not, then he answers two different questions. He asserts, first, that there are no relations and that acts in particular are properties. He asserts, second, that, psychologically speaking, one thinks of act-properties in a certain way, namely, the way in which one thinks of all his so-called relational properties. Hence he has not been able to show that one can deny the existence of relational acts and at the same time solve the problem of how selves are connected with other selves and independent material things.

University of Illinois

FROM SUCCESS TO TRUTH

By PETER ACHINSTEIN

IN ' Predicting and Inferring '[1] Ryle takes the position that there is no *inference* which is made from the successes of a theory to the truth of the theory, and hence no inductive inference of this kind. The scientist doesn't infer the truth of a theory that all A is B from the success of inferences of the form ' x is an A, so x is a B ', ' y is an A, so y is a B ', etc., which the theory generated. Rather, the success of these inferences *shows* that the theory works. And it does this by showing it working. A scientist does not argue, ' Because the inferences about x, y, and z worked, therefore the theory works.' Ryle concludes, ' . . . there is no question of there having to exist a special sort of argument, to be called an "inductive argument", *from* the successes and failures of the inferences *to* the truth of the theory. *A fortiori* there is no question of there having to exist a sort of inductive argument the cogency of which increases with the sheer multiplication of favourable instances, in the way in which the evidential value of a sample really does, in certain conditions, increase, up to a point, with increases in the ratio between its size and the size, if it has one, of the population of which it is a sample.'[2]

Ryle's claim, however, seems questionable for reasons which I shall try to explain.

If I want to show you that the engine in my old jalopy is now working, I pull out the choke, turn the starter key, put my foot on the accelerator, and wait for the shakes and rattles. In showing you this I am not making an inference to the fact that my car engine is now working nor am I putting before you the necessary facts from which you (and anyone else) can draw that inference. There's no question of an inference here because I am, so to speak, producing and putting before your very eyes the event or occurrence which I promised I would show you; I am exhibiting the actual working engine.

Suppose, however, I want to show you today that the engine in my jalopy will work in the race tomorrow. I draw your attention to the overhauling I have just done, to the new parts I have just installed, and to top it all off, I actually start the car, making you listen to the smooth hum. But, of course, I cannot today produce tomorrow's running engine. What I can do is point to certain features which give a good indication of tomorrow's engine-performance. And this means that an inference from the existence of these features to tomorrow's event is warranted. Now, by drawing your attention to these features I am not

[1] *Proceedings of the Ninth Symposium of the Colston Research Society* held at the University of Bristol, April 1957.

[2] *Ibid.*, p. 170.

thereby making an inference that the engine will work tomorrow, since drawing attention to something is not making an inference. My *showing* you that it will run tomorrow doesn't consist in making such an inference. But it does consist in putting before you the necessary facts from which you (and anyone else) can draw such an inference. If the inference can't be drawn, then I haven't shown you what I said I would. If there is no inference to be drawn from the facts which I have pointed out to you, then I cannot show you that the engine will work tomorrow.

Ryle says that we show that a theory works by pointing out that inferences which it generates have been successful. Now the question is, what does Ryle mean when he says of a theory that it *works*? He says, 'If the theory works, then *inter alia* the particular inferences made on the basis of it, if validly drawn from true premises, will be successful, *i.e.*, their conclusions will turn out to be true. This is part of what we mean by saying that the theory works.'[1] To say of a theory that it works entails, then, that the inferences which it generates will be successful, *i.e.*, that inferences with true premises which are drawn in accordance with the theory will have true conclusions. Therefore to *show* that a theory works involves showing that the inferences which it generates will be successful.

Now, if I want to show you that certain of the inferences which a particular theory generates have been successful, then I produce these particular inferences and point out that they have been successful, *i.e.*, that they were validly drawn in accordance with the theory, their premises were true, and their conclusions turned out to be true. In showing you that they are successful inferences generated by the theory I am not thereby inferring to their success, nor am I putting before you information from which you can infer to their success, since I am, in effect, producing these successful inferences themselves. This is like the case of showing you that my car engine is now running by pushing the starter.

But if I want to show you that the inferences which the theory generates *will be* successful I cannot now put before you the success of these inferences, just as I cannot now put before you tomorrow's running engine. What I do instead is point out inferences generated by the theory which have been successful; and this *shows* that other inferences generated by the theory will be successful only if you (and anyone else) can draw the inference that they will be successful, *i.e.*, only if such an inference is warranted. That is, what I do here, as in the case of tomorrow's running engine, is put before you certain facts from which a certain inference can be rightfully drawn.

We can go along with Ryle when he says that pointing to the success of a theory is not inferring to its success. And this is because pointing

[1] Ryle, p. 166.

to something is not inferring. But this does not mean that there is no inference or argument from the success of inferences to the claim that the theory generating them works, as Ryle suggests. For unless such an inference were warranted it could not be said that one had shown a theory to be successful, as Ryle is using this term. The scientist not only points to the past successes of his theory, but he infers to its future successes as well. And the more numerous the successful inferences which the theory has generated, and the more widely varied the conditions under which such inferences were successful, the more likely, he concludes, that all inferences generated by the theory will be successful. And if the scientist doesn't actually make or draw the inference, nevertheless the inference is warranted. For if not, then he hasn't *shown* that his theory works, *i.e.*, that it will be successful.

A word should be said about arguing to the *truth* of a theory from the successful inferences which it generates. Ryle rejects this as he does the notion of inferring that a theory *works* from the successful inferences which it generates. Arguing to the truth of the theory that all cats eat mice is, I suppose, arguing simply that all cats eat mice. Now on the basis of, or from, the success of the inferences ' Cathy is a cat, so Cathy eats mice,' ' Dolcie is a cat, so Dolcie eats mice ', ' Rubenstein is a cat, so Rubenstein eats mice ', I argue that all cats eat mice. And this is the same as arguing to the *truth* of the statement 'All cats eat mice ' on the basis of the success of the previous inferences.

Now it might be objected that the parallel between theories and car engines breaks down: for, where car engines can work one day and not the next, theories cannot. If a theory ceases to work because of an experiment on Saturday we most likely won't say that it worked from the beginning of the week and now does not; but rather, we should probably say, it *seemed* to work during this time. But we don't say this of the car engine. So, it might be concluded, although it makes perfectly good sense to infer that my car engine will work next week from its fine performance to date, it doesn't make such good sense to infer that a certain theory will work next week from its performance to date.

I suggest that again we look at this word ' works '. If I say, ' My car engine works ', I may be taken to mean that it works now and will work in the immediate future. Do I imply that it will be working in two weeks, two months? Probably not. In these cases I should have to add, 'And it will be working in two weeks, two months '. But when I tell you that theory T works, I am not saying that it has worked up to Saturday and will probably work through next week, but after that, who knows. Am I therefore saying that it has worked up to Saturday and will therefore always work? One could say this, if it weren't for the misleading phrase, ' will always work ', which suggests that theories are the sorts of things that can work up to a particular time and then

stop, like clocks and engines. The point is, when we say that a theory works we imply that the particular inferences made on the basis of it will be successful. And this implies among other things that the conclusions of these inferences will turn out to be true. ' The engine works ' tells us what will happen now if we pull out the choke and push in the starter. ' This theory works ' tells us what will happen now if certain conditions obtain, and what will happen *at any time* if these conditions obtain. And since it tells us the latter, an inference must be warranted from what happens (and has happened) when certain conditions obtain to what will happen when these conditions obtain. And we can call this an inference from the fact that a theory has worked to the claim that it will continue to do so, or an inference from the successful inferences which a theory has yielded to the claim that it works, provided we aren't misled into thinking that theories are the sorts of things that can eventually wear out. So, if we understand this, the analogy would not appear to be an unfair one.

Harvard University

COMMENT ON MR. ACHINSTEIN'S PAPER

By GILBERT RYLE

AN engine works if it runs properly; that is, if it ticks over, accelerates, etc., as it was designed to do. The engine can be working now, i.e., running properly now; and it can have ceased to run properly by tomorrow. But an engine-*design* works if it is a successful design; that is, if engines made according to it run properly. The design does not itself run properly or badly; and it does not cease or continue to run. For it does not run—or stall either. It works, but it cannot be working *now* or *still* or *again* or *intermittently* or *no longer*. So let us drop my slippery verb ' work '.

We can wonder whether this engine that is running properly now will run properly tomorrow, and we can have evidence that it will do so, since we may have eliminated stoppage-causes. But can we wonder whether an engine-*design* which has been successful hitherto will begin to be unsuccessful tomorrow? Or can we want evidence that a *soufflé*-recipe according to which good *soufflés* have been made hitherto will by tomorrow have ceased telling us a way to make good *soufflés*? What sorts of ceasings would we be dreading, or what sorts of continuings would we be praying for? What kinds of stoppage-causes should we try to eliminate? For what future-tense conclusions about it should we now be collecting present-tense and past-tense evidence? We learn that a design or a recipe is a good one by finding that when the

prescribed materials are treated in the precribed ways, the resultant engines run properly and the resultant *soufflés* froth up properly. But these runnings and frothings are not clues pointing more or less problematically to the excellence of the design or recipe. They *are* its successes. They are particular dated fulfilments of its tense-general promise.

In debates about Induction we are not concerned with engine-designs or *soufflé*-recipes, i.e., with prescriptions how to construct objects with desiderated properties. We are concerned with natural laws, general hypotheses or scientific theories. These teach us, *inter alia*, to make, not things, like engines that run or puddings that froth, but factual predictive and retrodictive inferences that come off, i.e., issue in true conclusions, when (a) their factual premises are true and (b) their channellings are as prescribed. To say that a law or theory 'works' or is successful is to say, *inter alia*, that factual inferences made as it prescribes come off. These are the particular dated fulfilments of its tense-general promise.

Achinstein wants there to be an *inference* from the fact that factual inferences of a certain class have come off to the truth of their law or theory, and so to the future comings-off of further factual inferences of this class. This higher-level inference would, I take it, be that hallowed Inductive Inference of which the credentials—but not the existence—have been so strenuously canvassed. I think that Achinstein wants to assimilate this putative higher-level inference partly to the mechanic's prediction that this engine will run properly tomorrow and partly to the seedsman's or to Mr. Gallup's extrapolations from samples to populations.

I have no general grumbles with either of these considerably different kinds of factual inference. Such inferences can be bungled; but this itself entails that they need not be bungled. But I do grumble at the idea that there has to be an ulterior kind of inference *from* the occurrence of factual inferences that come off *to* the truth of their law or theory. We learn or discover the merits and demerits of an engine-design or a *soufflé*-recipe by making test-engines and test-*soufflés* and seeing whether these do run properly and froth properly. But learning is not, save *per accidens*, inferring; and learning to . . . is never inferring, not even when it is learning when we may and when we may not infer from what to what. We learn or discover the merits and demerits of general inference-formulae, like laws, by tentatively making particular factual inferences according to them, and seeing whether they come off or not, and under what conditions they do or do not come off. But this learning is not inferring, any more than learning to construe the Latin subjunctive is inferring. If we like jargon, we can call this learning-by-trying-out '*induction*'. But 'inductive *inference*' is a *vox nihili*, unless used—as it is

sometimes confusingly used—to denote the concrete factual predictions and retrodictions that the mechanic, the seedsman, the palaeontologist and the astronomer make, when they have learned to make them.

Achinstein acknowledges that 'is working now' and 'may not work tomorrow' apply to an engine in a way in which they do not apply to a law or theory (or, I think he would grant, to an engine-design or a *soufflé*-recipe). But he still hankers for some present-tense evidence for some future-tense conclusion about the law or theory holding good tomorrow, i.e., generating good factual inferences tomorrow. It is as though he thinks a law or theory is really a sort of Gallup forecast—a poll-summary published before the election. But a law is not a proleptic résumé of a lot of true, singular predications. So the question how we can anticipate these predications does not arise. To put it crudely, what a law tells us is, *inter alia*, that facts of certain specifications are evidence, of a certain quality, for conclusions of certain specifications. Addicts of the notion of inductive inference want there to be evidence, of an ulterior kind, in favour of these facts being evidence for these conclusions. But finding out that X is evidence for Y cannot always require first finding evidence Z for {X's being evidence for Y}.

Else nothing would ever *be* evidence. We do find out that some things are evidence for some other things; but this finding out is not always finding out by inference from ulterior evidence.

LOGICAL INDETERMINACY AND FREEWILL

By C. J. F. WILLIAMS

IN his paper 'On the Logical Indeterminacy of a Free Choice' (*Mind* lxix, No. 273 (January 1960) pp. 31–40) Mr. D. M. MacKay argues that since it is logically impossible for an agent to possess sufficient evidence to be able to predict his own free actions, the truth or falsity of such predictions is *for him* undetermined. The introduction of quasi-scientific terms such as 'brain processes' is not, I believe, essential to the argument. All that is necessary is to assume that from certain facts about a person it is possible to infer what he will do in a particular case, and that these facts include his own beliefs, if any, about what he will do in the case in question. The logical impossibility consists in the appearance of these latter facts both in the premisses and in the conclusion of the inference.

So far Mr. MacKay's argument is sound. In Section 3, however, he goes on to say that since it is logically impossible for the prediction of a free action to be known by its agent, no such prediction can be absolutely certain. Its certainty, he says, 'is only a conditional certainty—a certainty whose value varies according to the person contemplating the prediction.' And this is true, not only of the prediction that A will do X, but also of the conditional prediction 'Provided A does not get to hear of this, he is certain to do X'. This proposition, Mr. MacKay claims, 'will have a different truth-value for A and for the silent onlookers' (pp. 33, 34).

Consider the sentence 'A is mistaken in thinking *p*'. This expresses a proposition which it is impossible for A himself to express. For if A *knew* that he was mistaken in thinking *p* he would not be thinking *p*, and whatever he really believed, he could not *say* both that *p* is false and that he thinks *p*. The statement 'A knows that he is mistaken in thinking *p*' is self-contradictory. To be sure, there is no contradiction in saying 'A knows that he *was* mistaken in thinking *p*' when the time of the knowing and the time of being mistaken are different. (Compare Mr. MacKay's account, p. 38, *n.*, of how in retrospect the agent can join the onlookers and share in their outside view of his physical past as 'determined'.) Nevertheless, the fact that A is mistaken in thinking *p* is a fact which A, while he is still so mistaken, *cannot* know.

Does this mean that we cannot attribute certainty to the proposition that A is mistaken in thinking *p*, although there are people who are certainly right to maintain this proposition? Can it never be certain that anyone is mistaken, but only that such a description of the state of affairs is true 'to the onlookers'? Are statements about false belief

incapable of being ' universally true ' and doomed to ' logical indeterminacy ' ? (*cf.* pp. 33, 5.)

The doctrine that truth and falsity belong to propositions—which doctrine Mr. MacKay is calling in question—is not an empirical hypothesis but a way of drawing attention to the meaning of the words ' true ', ' false ' and ' proposition '. Since any definition of certainty will include the word ' true ', and allow ' certain ' to apply to the same sort of thing as ' true ' applies to, certainty also will be found to ' belong to propositions '. ' John kicked James ' said by Thomas is the same proposition as ' I kicked James ' said by John, but not the same as ' I kicked James ' said by Thomas. One use of the term ' proposition ' is to shew how different sentences can agree in respect of truth, or the same sentence differ in respect of truth when uttered by different speakers. The question of whether the proposition expressed by a sentence is true or not is in no way altered by the identity of the man who uttered the sentence. Where such matters of identity are in question we do not speak of ' propositions ' but of ' sentences ' or ' utterances '.

Mr. MacKay has shewn that the proposition expressed by the sentence ' Provided A does not get to hear of this he is certain to do X ' cannot be expressed by A. He may be right to consider it improper to talk of A's ' ignorance ' in this case. We do not say that a man who is mistaken is also ignorant of the fact that he is mistaken (though philosophers have talked about a man's knowing that he knows). Such remarks are improper because they are redundant. Nevertheless, it is precisely this improper sort of ignorance which is at the bottom of this alleged ' logical indeterminacy '. These know-all neuro-physiologists are able to say, on Mr. MacKay's assumption, 'A is certain to do X and does not know that he is certain to do X ', but not, *tout court*, 'A is certain to do X '. They are able to assert the conjunction but not the simple statement. A, however, is unable to assert the conjunction, because he is unable to assert that he is ignorant of the fact that he will do X *and* that he will do it, for this would be at once to assert something and to assert ignorance of it. This is little different, if at all, from A's inability to assert p and that he believes p to be false. This latter inability, however, is none other than the impossibility of a man's asserting his own contemporary mistakenness, which we discussed previously.

If the proposition that A will do X is true for anybody it is true for everybody, even for those who cannot express it. The idea that a proposition can be true at one time and false at another has come in for much recent discussion. The consensus of opinion seems to be that the phrase ' true *at time t* ' is meaningless. Equally meaningless, it seems to me, is the phrase ' true *for A* ', or ' true *for the onlookers* '. It is on the validity of these expressions that Mr. MacKay's fascinating argument, unfortunately, rests.

Balliol College, Oxford.

TYPES AND FORMATION RULES: A NOTE ON TRACTATUS 3.334

By EDWIN B. ALLAIRE

IN a recent issue of this journal Bergmann[1] has argued that the propositional-function notation is both 'useless and harmful'. It is useless because it serves no real purpose. What can be said with it can be said as well without it. It is harmful because it lends specious plausibility to two errors, namely, that certain *bad* arguments for nominalism are *good* arguments, and that sentences are names. In this note I wish to extend Bergmann's analysis by pointing to still two other harmful effects of the notation. They are: (*A*) the erroneous belief that the formation rules follow from the type distinction, and (*B*) the confusion the notation introduces into the discussion of 'logical form'.

A. Specifying a schema consists of two distinct steps. (1) Signs are divided into 'types' by dividing them into classes by shape. (2) Formation rules specify how signs may be strung together into sentences. These rules (2) do not follow from that classification (1). That is what I propose to show.

Consider a schema, L, containing signs of three shapes; x, f, and F. The instances of each are distinguished by numerical subscripts. There are two rules for making sentences; first, by juxtaposing an x-shape sign and an f-shape sign; second, by juxtaposing an f-shape sign and an F-shape sign. In no sense do the formation rules of L *follow from* its type distinctions, even though the former depend upon the latter in that in the statement of the former the latter are mentioned. On the other hand, there is nothing about the type distinctions *as such* which could in any way be interpreted as prohibiting the formulation of rules by which, say, '$f_1 f_2$' or '$F_1 x_1$' would be a sentence. That those strings are not sentences of L is due solely to their not conforming to the specific rules we have chosen for it. Thus, if one specifies the syntax of L without employing the propositional-function notation, the formation rules patently do not follow from the type distinction. Nor is one tempted to believe that they do. The function notation, on the other hand, does hold out such a temptation. Let us see.

> 3.332 No proposition can say anything about itself, because the propositional sign cannot be contained in itself (that is the 'whole theory of types').
>
> 3.333 A function cannot be its own argument, because the functional sign already contains the prototype of its own argument and it cannot contain itself. If, for example, we suppose that the function

[1] Gustav Bergmann, 'Propositional Functions', ANALYSIS, 17.2 (December 1956), pp. 43–48.

> $F(fx)$ could be its own argument, then there would be the proposition '$F(F(fx))$', and in this the outer function F and the inner function F must have different meanings; for the inner has the form $\phi(fx)$, the outer the form $\psi(\phi(fx))$. Common to both functions is only the letter 'F', which by itself signifies nothing. This is at once clear, if instead of '$F(F(u))$' we write: '$(\exists\phi):F(\phi u).\ \phi u=Fu$'.
> Herewith Russell's paradox vanishes.

> 3.334 The rules of logical syntax must follow of themselves, if only we know how every single sign signifies.
>
> .....

> 3.344 What signifies in the symbol is what is common to all those symbols by which it can be replaced according to the rules of logical syntax.

3.344 merely says that what matters is not the specific shape of a sign but only the type to which it belongs. 3.334 makes the claim: The rules I called (2) follow from the classification I called (1). 3.333 is the argument that supports the claim.

Call the schema to which 3.333 implicitly appeals, L′. L′ contains 'signs' of three shapes; $F(f(x))$, $f(x)$, and x. Its instances, like L's, may be distinguished by numerical subscripts; thus, '$F_1(f(x))$', '$f_1(x)$', and 'x_1'. In contrast to L, L′ *seems* to have no rules for making sentences. They are absorbed and therefore deemed to follow from the distinctions amongst the shapes, specifically, from the '$f(x)$' in '$F_1(f(x))$' and the 'x' in '$f_1(x)$'. Putting the matter that way makes the error transparent. What prohibits, say, 'x_1x_1' or '$f_1(f_1(x_1))$' from being a sentence? Nothing. The only reason why that is overlooked is that the formation rule is implicit not in the type distinction itself but in the propositional-function notation for it. Wittgenstein himself is forced to say that 'the function sign already contains the prototype of its own argument and it cannot contain itself.' That merely states a specific formation rule. The function sign itself does not declare what can and cannot complete it to make a sentence. If the function sign is thought of *strictly* as a sign and not as a sentence form (i.e., an implicitly stated formation rule), then the 'x' in '$f_1(x)$' is merely a place marker, showing that some other sign must be placed there in order to make a sentence. (Even this is a formation rule, though an incomplete one.) It would be clearer, therefore, to write the function '$f_1(x)$' as '$f_1(\,)$'. The blank is just that, a blank. It in no way reveals what can or cannot fill it. When, however, we write '$f_1(x)$', we write more than just a sign. We write a sign *plus* a complete formation rule. That is how Wittgenstein is led to believe, mistakenly, that the type rule, which is a formation rule, follows from the type distinction itself.

B. Some philosophers believe that schemata such as L or L′ are suitable tools for philosophical analysis. Those who do, usually argue

that the *form* of the schema which 'pictures' the world, represents the world's *form*. Thus, they tend to explicate 'the world's *form*' by means of the syntactical form of the schema they believe 'pictures' the world.

Consider two such philosophers, one employing L; the other, L'. Assume now they are both requested to explicate what they mean by 'the *logical* form of an atomic fact'. Let the fact f_1x_1 be their paradigm. Both will say that the fact has two constituents which differ in kind (ontological form), as reflected by the different shapes (syntactical form) of the signs referring to them. The L-theorist is likely to go one step further. At least he is in a good position to point out that the two constituents stand in a peculiar relation which is represented by the juxtaposition of the two signs. That latter relation is an essential syntactical feature of the sentence. It embodies a formation rule. Thus, our L-theorist is likely to claim that, just as the (syntactical) form of the signs represents the (ontological) form of the objects, juxtaposition represents (syntactically) the (ontological) form of the fact, or, more precisely, a third feature of the 'logical form' of the fact. The L'-theorist is less likely to take this further step. He will be tempted to overlook what, according to the L-theorist, is an essential feature. His oversight, if it be one, has been caused or at least facilitated by his failure to distinguish properly between the rules I called (2) and the classification I called (1). This is what I set out to show under (*B*).

One cannot but wonder whether the early Wittgenstein's perplexity as to how the *form* of a fact (i.e., what I called the third feature of the 'logical form' of a fact) is represented stems from his following Frege in employing an L'-like schema. For he had no difficulty in telling us how the *form* of an object is represented (cf. 4.126).

State University of Iowa

TWO LOGICAL PROBLEMS AND A THEORY OF MEANING

By PRAVAS JIVAN CHAUDHURY

Problem 1

AN indicative sentence, if meaningful, must express a statement which must be either true or false. If the statement is to be true, what is stated must not only be the case, but must also be capable of being otherwise and thus of falsifying the statement. We cannot know what it is for a statement to be true unless we know also what it is for it to be false. So that a true statement must be falsifiable though it is in fact not falsified. In other words, if there is a true statement it is logically possible for its contrary statement to be true and, so, to falsify it. Now consider a mathematical sentence like " $2+3=5$ " which appears to be indicative, and so to express a statement. But the statement, though known to be true, appears to be unfalsifiable, for if we try to imagine a contrary statement, expressed by, say, " $2+3=6$ ", to be true, falsifying the original, we cannot succeed. This is because what the new sentence expresses is not a statement contrary to the original, but one quite different from it. The numerals of the new sentence, 2, 3 and 6, do not now mean what they meant in the original sentence. For their meanings are determined by a different number-system or arithmetic to which the sentence may belong. So that the mathematical sentence in question, *viz.* " $2+3=5$ ", expresses a statement that is unfalsifiable, and so neither true nor false. But this means that the sentence does not express a statement; though it may express something else, such as a proposal.

Solution: The statement *primarily* (or directly) expressed by the sentence " $2+3=5$ " relates to a linguistic fact, to a relation of symbols. It states an equivalence between " $2+3$ " and " 5 ". But this relation of symbols depends on and illustrates a rule or convention for manipulating certain numerical symbols. This convention, formulated as a statement, is the *secondary* (indirect) meaning of the sentence. Since the statement of the linguistic fact is a logical consequence of the statement of the convention, it is logically impossible to falsify it if the latter holds, and it is also logically impossible to falsify it by stating some other convention which is logically unrelated to it, so that the statement in question does seem to be unfalsifiable in principle. But we may now enquire whether the rule or convention concerned is accepted for general use or not; that is, whether the statement asserting it describes an actual human behaviour with mathematical symbols or only a possible one. If it describes actual behaviour, then the statement is true, if not, it is false. So that " $2+3=5$ " does express a true statement because it implicitly describes some actual human behaviour with symbols and

because it could be false if the behaviour were otherwise. Hence we can now say in answer to our problem that " $2+3=5$ " expresses two different statements; one is analytic, and so unfalsifiable, while another is not analytic and is falsifiable.

Comments: (1) Acceptance and rejection of a mathematical system is not a matter of purely arbitrary choice or decision on the part of men, but of the applicability of the system for descriptive purposes. As so applied, the system is physically interpreted, that is, the symbols are correlated with certain observable features of the world and the rules for manipulating them are determined by and reflect the behaviour of actual objects. It is because of this that the system becomes widely accepted and called the " true " system while its possible alternatives are treated as purely logical games. So that although a mathematical sentence means *secondarily* a statement of a set of conventions, or rules of human behaviour, regarding the use of certain symbols, it *tertiarily* means a statement of a set of rules for the behaviour of natural phenomena in a certain field of enquiry. In other words, it not only implies or illustrates laws governing the use of certain symbols but also empirical laws of the physical world.

(2) This way of looking at the matter may be helpful in understanding the causes of, and resolving, the controversy over the question whether a mathematical statement expressed by a sentence like " $2+3=5$ " is analytic or synthetic; and, if synthetic, whether it refers to a fact of human behaviour with symbols or some non-human physical situation. My analysis of meaning in terms of meaning-strata shows that all the rival views in the controversy are partially correct and that they may be reconciled under a more comprehensive view of the matter.

(3) Some elucidation of the three strata of meaning of a mathematical sentence is required. The primary meaning is not usually recognized as primary; perhaps only a child who first manipulates the numerical symbols mechanically, while doing his arithmetical sums, means by them nothing else than the numerals. The physical interpretation of these symbols in terms of classes of things, what we have called their tertiary meaning, comes to acquire the status of the direct and, so, primary meaning just as the original etymological, literal meaning of a word or phrase may be obscured and superseded by the metaphorical one. Similarly, the secondary meaning of a mathematical sentence may be eclipsed by its tertiary meaning. " $2+3=5$ " states, implicitly, that the symbols concerned are thus manipulated at present in the culture group presupposed by the discussion, and this statement is true if what it asserts is the case. This is not perhaps what we usually mean by the meaning of a mathematical sentence and the truth of a mathematical statement. My use of the words " primary " and " secondary " is a

proposal which, if accepted, will help one to solve certain logical problems. The tertiary meaning of a mathematical sentence is often regarded as the primary one. This confusion has led to empirical and psychological accounts of mathematical judgments which treat them as exclusively synthetic. My proposed distinction between the levels of meaning relates the different modes of accounting for and resolving the disputes mentioned here.

(4) (a) If a mathematical sentence has a primary meaning only, that is, if it states only a linguistic fact that does not depend on and illustrate any convention about symbols, then what it expresses is a definition or a proposal. It proposes a convention or rule. Thus "$p \times p = p^2$" is a definition of "square" and the sentence is equivalent to "let any quantity multiplied by itself be said to yield its square".

(b) If a mathematical sentence like "$2+3=6$" has a primary and a secondary meaning only—the secondary meaning consisting in the statement of a convention about the manipulation of some symbols, which the statement primarily expressed by the sentence depends on and illustrates, but which, however, has not been established in our usage because it is inapplicable for world-description, and so has no empirical import—then what it expresses is unfalsifiable and so it is neither true nor false in the ordinary (material) sense. Yet it is a statement, though an analytic one, and not a proposal. It does not itself propose the bare convention but states a logical consequence of the latter being accepted. It describes a certain possible behaviour of man with symbols.

(c) A mathematical sentence like "$2+3=5$" differs from one of the sort described above in that it does, while the other did not, secondarily express a statement describing actual human behaviour with some symbols, and, tertiarily, a statement describing non-human physical behaviour. Therefore, although it may be treated as expressing an analytic statement by overlooking its tertiary meaning, it is more importantly a synthetic statement as the tertiary meaning is more important than the secondary.

(d) The question whether "Tully is Cicero" be called a sentence expressing an analytic or synthetic statement or a mere definition (proposal) may be answered in this way. Considering its *primary* meaning only, the sentence expresses a proposal equivalent to "Let us use the names 'Tully' and 'Cicero' interchangeably". In its *secondary* meaning it may be said to express an analytic statement, for it may be said to state the equivalance of these names on the basis of an actual or possible linguistic rule that "Tully" and "Cicero" are to be used indifferently. But so far as the rule may be said to have been stated by the sentence itself, the primary meaning of which does not depend on and illustrate the rule, it is more appropriate to hold that the sentence expresses a

proposal than an (analytic) statement such as is expressed by " $2+3=5$ ", which does depend on and illustrate a set of number rules. But if the secondary meaning of the sentence is that Tully and Cicero are equivalent as names because people so use them, then it refers to a contingent fact about the human use of names which might be otherwise. As such it is a synthetic statement. Since the names are proper names and refer to an identical and unique object instead of a general characteristic they cannot be said to describe anything in the world. So that there seems to be no tertiary meaning of the sentence. We cannot mean to give by the sentence any information about the person Cicero or Tully, we can only inform about the use of the same name for the same person. Of course, we may have to mention some characteristics to identify the person referred to by the names but these are unspecified and loosely connected with the names which are not elaborate definite descriptions replaceable by any such set of characteristics. But one may easily overlook this crucial difference and think that a name refers to an object by virtue of a set of characteristics it means, and so may think the sentence " Tully is Cicero " to have a *tertiary* meaning over and above its primary and the secondary ones. We can thus understand and resolve the controversy over this question.

Problem 2. Every empirical statement is falsifiable in principle. But suppose we find a metal not conducting electricity, then is the statement " Metals are good conductors of electricity " falsified? Surely not, for we will not call that particular non-conducting substance a metal. So the statement is not falsifiable by any observation purporting to falsify it. Yet it is empirical in the sense that it refers to the facts of the world; and this suggests that the relations of facts are not contingent but necessary, and that facts, when truly known, entail one another like elements in a definitional system such as a system of logic or pure mathematics.

Solution: The question whether the non-conducting substance be called a metal or not will be decided by whether the characteristics which are common to this substance and conducting metals are more important than the characteristic in which they differ, namely conductivity. Now this is a decision to be made on the ground of convenience in the systematic description of phenomena in the field of enquiry to which the statement belongs. The scientist may find it advisable to call the substance a metal, though a particular kind of metal defined by its non-conductivity and perhaps by some other peculiar properties to be found later on associated with non-conductivity. So that the original empirical law that all metals are good conductors of electricity would be falsified. Thus it is a methodological question to be decided by the criterion of scientific concept-formation, viz. that the total linguistic system should be the simplest, most systematic and most comprehensive

possible. The attempt to save an empirical law from falsification by a logical device may have disadvantages in scientific concept-formation, the history of which shows many instances of scientific laws being revised. So scientific laws are empirical and the facts they describe are contingent.

Comments:

We may understand and explain this situation in the logic of science in terms of meaning-strata as explained above. A law-statement means *primarily* a linguistic or symbolic fact that depends on and illustrates an actual or possible human conventional behaviour (H) with some symbols to represent some actual or possible physical behaviour (P) in a certain field of investigation; H and P being respectively the *secondary* and the *tertiary* meanings of the statement. Now when a symbolic system or scientific theory becomes well-established, the truth of any particular statement following from that of others deductively, we tend to think of it as a deductive system, so that the tertiary meaning of each statement of the system, that is, the phenomena described by it, tends to be assimilated to a definitional symbolic system, by analogy with the secondary meaning. Since an accepted convention to use certain symbols in a certain manner explains why certain linguistic facts that depend on and illustrate it are necessarily what they are, so one tends to regard the tertiary meaning of a statement as an analogue of the secondary one to explain the constancy of the scientific system. One is reminded of the Aristotelian tradition in logic that supposed everything to have a real definition, fixed and final, the business of science being to reveal these definitions and the deductive system underlying the sensible world. So that the intuition of these definitions or true scientific principles and their deductive consequences must needs be the chief method of science; verification of these consequences by sensible facts and induction from them being of secondary importance, by way of suggesting and confirming the findings of our intuition. One is also reminded of Berkeley who thought the world to be Divine language; clouds meaning rain and fire meaning smoke, and the business of science to discover the grammar of this language. These are all consequences of construing the tertiary meaning-function of a sentence as a secondary one in order to fortify one's faith in certain necessary knowledge.

Presidency College, Calcutta

'CONTINUITY' CONTINUED

By G. C. NERLICH

MR. ROBERT C. COBURN[1] has 'certain misgivings' about arguments used by me, among others, in supporting the thesis that bodily continuity is a necessary condition of personal identity. Perhaps I can remove them by making it a little more plain in what context the arguments occur. I wish to make two points.

I. There are two cases involved. C1: A ceases to exist at T_1 and an exactly similar thing or person B begins to exist at T_2. C2: A ceases to exist at T_1 and two exactly similar things or persons begin to exist at T_2.[2] (Here and throughout, 'exactly similar things or persons' is used so as to leave open the possibility that the things may be identical; and for persons it includes their having similar convictions about the past.) C1 is introduced as a case where we should identify A and B but where the condition of their bodily continuity is absent. But we can only exclude a condition from being necessary for correctly applying a concept if we have a different set of conditions which are sufficient for its correct application. In claiming that we have in C1 a case of identity without continuity, one is claiming that the other conditions realized in C1 *are sufficient* for identity, and the only conditions realized here seem to be the mere similarity of A and B. C2 now shows the absurd consequences of taking these conditions as sufficient. The new arrivals B and C are the same person, etc. This absurdity emphasises the fact that the distinction between identity and exact similarity, at present a real one, becomes quite vacuous if we accept the conditions obtaining in C1 as sufficient. For 'identical' now means no more than 'exactly similar' means. (*This* is what is vacuous, not the assertion of identity, which is now the non-vacuous assertion of exact similarity.)

Coburn has quite missed this central point, unless I grossly misunderstand him. He claims that it is as paradoxical *not* to identify A and B in C1, as it is to identify A B and C in C2. (p. 118). But there is no *paradox* about it, as there is in C2; at most it is a surprise. And if we went on to act as Coburn thinks we would in C1, we would, surprisingly, be wrong—as Williams and I have argued. However the central question is what Coburn takes himself to *mean* when he asserts identity in C1. He is at pains to avoid saying that the exact similarity entails (i.e. is a sufficient condition for) identity,[3] but if he avoids saying that, then, as I see it, he avoids saying anything to the purpose. For the challenge is to produce the set of sufficient conditions which yields

[1] 'Bodily Continuity and Personal Identity', ANALYSIS, 20.5 (April 1960).
[2] See B. A. O. Williams, *Proceedings of the Aristotelian Society*, lvii (1956–57).
[3] *Ibid.*, p. 118.

identity, but leaves continuity out. (Presenting the challenge does not involve denying that similarity is powerful *evidence* for identity, in the world as it is.[1] If, when he asserts identity in C1, he does mean to assert more than exact similarity, then *what* more does he mean to assert? And which feature realized in C1 supports this more, whatever it is? What, after all, are the conditions being appealed to as sufficient? One wants to be quite sure that there is no unconscious appeal to a nebulous and confused continuity of A with B; to the mere disappearance and reappearance of A.

II. Coburn's failure to see the issue in this light explains, I think, why my reason for making some further claims eludes him, as he puts it. Now, that *in fact* only B comes into existence later, cannot be claimed as the further condition required to make the C1 conditions sufficient. For Mr. C. B. Martin proposed,[2] as a criterion for identity or non-identity in C1, that the *possibility* of A's having continued to exist until T_2 so as to be there when B begins to exist, would entail that they were not identical; that is, allowing that if A had continued to exist then it could later have been set beside B is allowing that A and B are different. Coburn claims that since we have a reason for asserting identity in C1, we have just the same reason for denying the possibility that A might have continued so as to exist beside B. That is, he makes the identity statement a condition of the falsity of the counterfactual conditional stating this possibility. But the identity statement in C1 is in question on *this* issue: does it assert more than that A and B are exactly similar things? All that is true in C1 *as so far described* are the following statements (set out on p. 146 of my article): An object exists from T_1 till T_2 when it ceases to exist. Later, at T_3, an object comes into existence exactly similar to the object existing from T_1 to T_2. Now, given *just this*, is the possibility open of there being two exactly similar objects existing at T_3, supposing that no object had ceased to exist at T_2? Well, obviously the logical possibility is open. There is no contradiction between the statements given in C1 and the counterfactual which states the possibility. So there is the logical possibility of side-by-sideness, and I'm inclined to think that this is all one really needs. But I also wanted to make the stronger claim that nothing rules the possibility out as a contingent or empirical one. When one considers it in this light, the situation is odd. For merely in setting up the case we have moved beyond the present natural world in which we make identity statements and count similarity as good evidence for them. We've broken the relevant natural laws, or perhaps better, in our imaginary world no natural laws of this world are relevant. The imaginary world must be

[1] I have argued this issue at some length in *The Australasian Journal of Philosophy*, 37.3 (December 1959).

[2] ANALYSIS, 18.4 (March 1958).

constructed so as to show how we would come to adopt in it some natural law which would rule out the empirical possibility of side-by-sideness. I claimed that there was no hope of this, despite the obscurity surrounding the question of the truth conditions of counterfactuals. I am at present less confident that this view is correct, though even if it is wrong the weaker position remains unaffected. Mr. Coburn was quite justified in complaining that I had failed to be explicit on this important point. But I think he was mistaken in claiming that *my* argument begged the question.

University of Leicester

EDITOR'S NOTE: CHANGE OF ADDRESS

The Editor's address is now as follows:—

Department of Philosophy,
The University,
Birmingham, 15.